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And here permit us to repeat, by way of explanation, that Darwinism does not mean the theory of development or derivation, pure and simple, as so many insist, but that explanation of its action by the law of natural selection which is given by Wallace and Darwin. We have no objections to urge against the theory which accounts for the origin of species by descent from some ancient and simpler forms, which might be appropriately called Lamarckianism, but only against the universality of the law of natural selection. This is applied to the solution of the origin of all the various modifications of form and characteristics which have arisen since the first appearance of life upon the globe, whereas it is evidently only a secondary law, active perhaps in all species but subordinated to some other and more comprehensive law still undiscovered.

As regards the origin of man himself our author takes the ground that "some higher intelligence may have directed the process by which the human race was developed by means of more subtle agencies than we are acquainted with.

At the same time I must confess, that this theory has the disadvantage of requiring the intervention of some distinct individual intelligence, to aid in the production of what we can hardly avoid considering as the ultimate aim and outcome of all organized existence—intellectual, ever-advancing, spiritual man. It therefore implies, that the great laws which govern the material universe were insufficient for his production, unless we consider (as we may fairly do) that the controlling action of such higher intelligences is a necessary part of those laws, just as the action of all surrounding organisms is one of the agencies in organic development. But even if my particular view should not be the true one, the difficulties I have put forward remain, and I think prove, that some more general and more fundamental law underlies that of "natural selection." The law of "unconscious intelligence" pervading all organic nature, put forth by Dr. Laycock and adopted by Mr. Murphy, is such a law; but to my mind it has the double disadvantage of being both unintelligible and incapable of any kind of proof. It is more probable, that the true law lies too deep for us to discover it; but there seems to me, to be ample indications that such a law does exist, and is probably connected with the absolute origin of life and organization.

In this connection read the original thoughts in the closing paragraphs on "The Nature of Matter," "Matter is Force," "All Force is probably Will-force," expressed in brief thus: "if, therefore, we have traced one force, however minute, to an origin in our own WILL, while we have no knowledge of any other primary cause of force, it does not seem an improbable conclusion that all force may be will-force; and thus, that the whole universe is, not merely dependent on, but actually *is*, the WILL of higher intelligences or of one Supreme Intelligence."

AMERICAN MICROSCOPES AND THEIR MERITS.* — The first of these papers is an elaborate attempt at an account of American microscopes and their merits; but should have more properly been entitled an attempt to describe the microscopes made by R. B. Tolles, as of the twenty-five pages which it covers, twenty are given to Tolles. The second article

* On the North American Microscope. By Dr. H. Hagen, Cambridge, Mass. Max Schultz's Archiv für Microscopische Anatomie. Bonn. 2d No. 1870. A communication by Dr. H. Hagen on his experience in the use of the microscope. Proceedings of the Boston Society of Natural History, vol. xii, p. 357. March 10th, 1869. A verbal communication on Tolles's and Scheick's microscopes, to the Boston Society of Natural History, November 10th, 1869. Unpublished.

above named, but first in time, is noticed here merely to illustrate some points in the first, and the third, because it was preliminary to the first, which only elaborates more in detail what Dr. Hagen said in his verbal communication, and repeats statements and assertions which at the time they were made, Dr. Hagen was informed, by those as fully competent, to say the least as himself, were erroneous; but in this first named paper Dr. Hagen sees fit to entirely ignore the refutations, and makes the same statements deliberately again, as though there had been no contradiction of them. There is no other course left for those who know him to be wrong or feel aggrieved by his statements, than to examine his qualifications for pronouncing judgment, and to show wherein he is mistaken.

Dr. Hagen being a man of acknowledged scientific acquirements, and holding a reputable position at Cambridge, his opinions, given on a professed detail of facts, and after a *claimed careful study* of two years, published in a journal of high repute in Europe, will command attention and respect there, among those who have no opportunity to see and judge for themselves. If he had stated facts correctly his paper might have been left to itself to refute his "opinions." No one can object to any comparison of American instruments with others; it is only asked that the comparison shall be made fairly, and by a competent expert. The writer proposes to show that Dr. Hagen's investigation has been superficial and inadequate to the task he undertook; and that he has mistaken facts and repeated assertions after he had been informed that they were erroneous.

Dr. Hagen opens his first communication to the Boston Society of Natural History by saying: "Having worked with the microscope more than thirty years for medical and scientific purposes—following the gradual perfecting of the instrument—I was anxious to examine the power [?] of American microscopes." This passage sets forth his claim to be a competent critic.

"During the past ten years there has been great competition among opticians, but in every case their progress has been arrested by one insurmountable obstacle." [What one?] "Since the *recent* improvement in correcting objectives for the thickness of covering glasses, comparatively little has been done." Why he should have restricted the "great competition" to the last ten years, and called the improvements in objectives "*recent*," when the competition in London has been active for forty years, and the "improvement" was made by Ross nearly or quite thirty years ago, can only be explained by supposing what has been generally believed to be the fact, that the "improvement" and the competition had not reached Germany until the last ten years. So far from little having been done since the "improvement" so much *has been done in England* that the London Microscopical Society, which procured objectives of the "three" leading London artists in about the year 1845, in 1867-8 abandoned the whole of them as behind the times, and obtained new ones of the same makers.

Dr. Hagen then makes some very just observations on "the difference in the aberration of the eyes of the observers. There is no doubt that different observers obtain different results with the same instrument." This is an important fact and an important admission from Dr. Hagen. It is well known to many microscopists, but is generally ignored. It is a pity that it did not occur to Dr. Hagen to remember what he had written in March, when he in October recorded some of his own observations.

The paper in the "Archiv" begins by saying for the past twenty years that the "prominent excellence of American microscopes have been frequently mentioned" and it has been "asserted that their achievements have essentially excelled those of European make." "To my knowledge a direct proof of this has never been exhibited, it has not been shown that anything has been ever better seen than with European instruments." "Thus the American instrument constituted until recently a myth towards which all interested in this branch of science gazed with anxious curiosity, and prompted me during my two years residence in this country, to become *thoroughly acquainted* with it, and I have *spared no pains* to study them carefully." Here we have distinctly the task set forth, and the claim that he spared no pains to accomplish it. Two years of the spare time of a busy man was rather short for the undertaking, especially for one with an imperfect knowledge of the English language. Let us see what were the "pains" taken. "The members of the microscopical section of the Boston Society of Natural History, especially Mr. Bicknell of Salem, Mr. Greenleaf of Boston, Professors Agassiz and Gibbs, Mr. Edwards of New York, and Mr. Tolles himself, have kindly seconded my efforts." Four of these gentlemen certainly were competent to assist. The writer cannot say what Mr. Edwards or Professors Agassiz and Gibbs did for assistance; but he states positively that neither Mr. Greenleaf or Tolles "assisted;" that Mr. Bicknell was the only one of the three who had any intimation whatever of Dr. Hagen's intention of becoming "thoroughly acquainted" with the American microscope, for the purpose of publication; they were never *asked to assist* for any such purpose. Had Dr. Hagen not spared his "pains;" had he enquired for those who could have "assisted" him in his "study" and have given him "positive proofs," he would have been referred to Professor Holmes and Professor Bacon of his own university, and to Professor Smith of Hobart College, New York — *Microscopists* who have made a study of the microscope for twenty years — to Dr. Barnard, Pres. Columbia College, New York; to Professor H. J. Clarke of the Kentucky University; to J. E. Gavit, Esq., of New York; to Dr. F. W. Lewis of Philadelphia; to Professor C. Johnston of Baltimore, to Mr. J. S. C. Greene, jr., of Boston; gentlemen who have made the comparison of European microscopes of the *best makers*, with American instruments almost a specialty; had he done this his study might have produced more correct results; that is if he had given heed to the information he received — for he seems to have disregarded that which he obtained from Messrs. Greenleaf and Bicknell.

Dr. Hagen gives his "general opinion" before giving the details, and says "novelty of any importance is not obtained." Yet before he concludes his paper he enumerates six novelties, all invented or designed by Tolles, namely: his binocular eye-piece; the illuminator of opaque objects with high powers; the low power immersion lens; the solid eye-piece; the mode of effecting adjustment for covering glass, and the amplifier; and overlooks others quite important by Tolles and Zentmeyer.

"Objectives and oculars accomplish with slight variations as much as the best European, never more; on the contrary English and French objectives have accomplished some things which the American have hitherto failed to do." It is not the purpose of this paper to produce evidence outside of Dr. Hagen's own statements, as to what American objectives have done. It is only needful to contrast what he says above with what he says he himself saw. Dr. H. says "that an objective 1-10th inch with ocular C. showed while band 19 [of the Nobert test plate] was in the centre of the field, the 18th, 17th, and half of the 16th bands; the lines in all were *well defined*, but not so that I could have counted them all. I could *count about forty* of the 19th, the rest blurred." "None of Tolles' objectives have well resolved the 16th to 19th bands of Nobert's plates which has been done with the 1-16th of Powell and Lealand." It would seem incredible that the same person could have written the above lines in the same paper; most especially after he had been positively informed by five gentlemen that they had seen the 19th band resolved, and with *several* of Tolles' objectives. But Dr. Hagen takes the ground (though not in this paper, as he should have done) that because *he* did not *count all the lines* at once, that they were not resolved; and it is true that he is not alone in that theory. To show the absurdity of this we will suppose that Nobert had ruled in the 19th band only 28 lines instead of 57, would Dr. Hagen say they were not resolved, when he saw the whole, because there were no more? Or if Nobert had covered a whole inch with the 112,000 and some odd lines, would any one claim that they must all be seen at once? If either of these suggestions are answered in the negative, then Dr. Hagen has himself seen the 19th band resolved with a Tolles' objective. But Dr. Hagen says that American objectives have done "never more than European," and yet what he did with a 1-10th objective, is much "more" than to see all the lines with a 1-16 (really a 1-20). He never saw, read of, or heard of a 1-10 European objective that would do what that one accomplished. This is not all; his sight of the *Surirella gemma* gives the same contradiction to his "opinion." He says "*S. gemma* with the same 1-10 showed only in a few places oblong fields between the cross lines, but not well defined or regular as in Hartnack's drawings." Well, did any one ever see them so? If Dr. Hagen knew as much of diatoms as of insects, he would have been aware of the fact that Hartnack's figure is a theoretical diagram, not a representation of the appearance in the microscope. Probably the only person living who claims to have seen what Hartnack calls the "flat hexagons,"

is Mr. Bicknell, who says he saw them, and only with a Tolles' 1-12. Hartnack does not say distinctly that he has seen them with a 1-16; he attempted to show them to two accomplished microscopists, and both failed to see them. Dr. Eulenstein has also failed with Hartnack's Nos. 10, 11 and 12, Powell and Lealand's 1-50 and Ross' objectives; and Dr. Hagen knew these facts, for the writer told him before his paper was written; comment is unnecessary. Dr. Hagen also says that Hartnack's 1-16 has resolved *S. gemma*, and Tolles' 1-10 has not, ergo Hartnack's has done what Tolles' could not. Dr. Hagen has himself furnished the "direct proof" he wanted of the "unsurpassed excellence" of the American objective.

Now for some of Dr. Hagen's errors and mistakes. He says of Tolles' objectives "the workmanship is superb," "the adjustment only moves the lower lens from the two others." The solid eye-pieces are "really bi-convex Coddington lenses." He gives on the authority of Edwards a formula of Tolles' objectives; all there is to be said, is, that the formula is not Tolles' formula, the eye-pieces are not Coddington lenses, and that Tolles had never made objectives to move the front lens; all of which Dr. Hagen could have easily ascertained.

Dr. Hagen considers that "a most important fault of the instrument consists in the difficulty of its use. In order to adjust them so that they will give their greatest results requires delicate labor and considerable time. In this respect they are excelled by the higher as well as the lower powers of English and German." "The ease of treatment of Hartnack's and Scheick's highest objectives is certainly far less troublesome." If this means anything it must refer to the delicacy of the adjustment for covering glass. Undoubtedly Scheick's are far less troublesome. It is thought to be well known to microscopists that the delicacy of this adjustment — consequently in one sense the difficulty of use — is increased just in proportion to the approach to perfection of the lenses. Certain it is that Hartnack when delivering an objective made for a member of the Boston Society of Natural History two years ago, called the purchaser's attention especially to the fact that when an object was best shown, the movement of the adjusting ring one hundredth of an inch either way destroyed the effect, as an evidence of the perfection of his work. As to English objectives. Dr. Piggott in a recently published article on high power objectives, speaks of a certain effect being entirely destroyed by a change of this adjustment which moved the lens only 1-14,000 of an inch. So much for English lens and Hartnack's. Microscopists know that Dr. Hagen is in an error as to good objectives, but correct if his remarks are applied to poor ones; and it is not surprising that he was "*utterly astonished* to see how much more the hand of the artist himself will develop with the instrument."

The majority of the microscopists here are "dilletanti or workers on diatoms;" this must be news to Professors Holmes, Bacon, Ellis and Gray, and to their hundreds of past and present students; the "truth will be

respected" if it is said that there are hardly enough diatomists in the whole country to encourage each other.

Dr. Hagen thinks that his attempt at "even pronouncing a judgment on the local instruments, caused a storm of indignation against me by the resident microscopists," and accounts for it by the assertion that "*we know* that most of them are members of the Boston Optical Association." Dr. Hagen here refers to the reception of his verbal communication to the Boston Society of Natural History in November last. Of all the persons then present but two were members of that association, and whatever indignation was manifested was at his preposterous comparisons of cost. Dr. Hagen then asserted that the American instruments cost 600 per cent. more than German of equal merit, and that "English objectives of the most celebrated makers could be imported to advantage." In his paper in the "Archiv" Dr. Hagen reduces the comparative cost of German and French objectives to "one-third or one-fourth as much," but repeats his comparison as to the English "according to Frey's statement." Now before this paper was written the cost of importing English objectives was read in detail to Dr. Hagen, and it was shown from the makers' price lists that the cost was much higher than Tolles' prices for similar objectives, and yet Dr. Hagen elects to repeat his erroneous statement. He said then that he "spoke for the interest of science." Can the interest of science be promoted by such misstatements? It was not the intention of the writer to have said anything more on the matter of cost, but while writing this paper a letter was received, an extract from which is a good comment on all that Dr. Hagen has said as to cost and workmanship. It is not known that the writer of the letter ever heard of Dr. Hagen or his comparisons. The letter was written by Colonel J. G. F. Holston, M.D., Washington, D.C., June, 1870. "I was never dissatisfied either with Tolles' prices or his workmanship, for although apparently dearer than some other makers, the superior excellency more than balances it. I can do with my 1-12th by Tolles (cost \$100), all that Powell and Lealand's 1-50th will do well that cost the United States \$350. I compared them myself at the museum." Dr. Barnard, President of Columbia College, New York, writes, "Dr. Hagen is absurdly wrong in his comparison of the performance of the American and foreign objectives of the same price." "It is nonsense to make such comparisons as these price for price."

No less unfortunate is Dr. Hagen in his description of Tolles' first class instruments; he partially describes the plan and construction of some instruments which he had seen — omitting, however, some of the most peculiar details — and mixing with that some of the peculiarities of an unique instrument, the only one of the kind ever made, and which he has never seen, the particulars of which he could have got from Dr. Barnard's report of the Paris Exposition of 1867 — constructing in this way an instrument which has no existence. He claims to have "seen and tested nine of Tolles' instruments of the largest class." The writer will

not say that is impossible, but he can say that there are no nine instruments of the "largest class" known to Mr. Tolles that Dr. Hagen could possibly have seen and tested. His classification must be treated as an error until he furnishes a list of the nine. The self-sufficiency with which he charges the reverend President of Columbia College with making, in his official report of the Paris Exposition, a claim that is "hardly tenable" is, to use his own expression, "quite comical." Dr. Barnard had reported that "it was to be regretted that the American makers did not send" stands to the exhibition; for the want of them the objectives were not properly examined. Dr. Hagen twists this round in this way. "The same objectives are frequently used here with English stands and oculars, plenty of which were to be had in Paris. If, then, they did not prove themselves successful the *reason must be* that they did not attain as much as others. The circumstances of the difficulty of their adjustment is not to be allowed in this case as the reporter (Barnard) himself is an adept in their use," all of which is entirely imaginary with Dr. Hagen. A recent letter from Dr. Barnard recites the whole story. He says: "In regard to what Dr. Hagen says of my report, he so singularly misunderstands me, or so wilfully misrepresents that it seems hardly necessary to reply to him. I never said or intimated that a Tolles' stand was necessary to develop a Tolles' objective, but only that a stand of some kind was necessary, a proposition which I think stands to reason. The disadvantage could not appear until the jury, instead of examining the glasses, country by country, as I supposed they would, using certain uniform tests, ordered at once all the exhibitors of microscope objectives to present their glasses simultaneously in one place (and that, by the way, as bad a place as could be selected, a small room with one window, a moderately sized table, and no chairs). Had the first plan been pursued there would have been no trouble about stands, for Mr. Beck of London was close by the American section with a case full of apparatus, including stands of all forms, one of which he subsequently placed at my disposal for some length of time. But when the crowd came together at the place appointed, the American glasses were present without any stands, and though both Mr. Ross and Mr. Beck, *after their own glasses had been examined*, permitted me to make use of their stands, the weariness of the protracted examination, with the extreme heat of the crowded room, made the jury impatient, and notwithstanding the compliment Dr. Hagen pays me as an "adept," I was not smart enough to secure, on that occasion, what I thought a fair trial of the glasses — by which expression I mean *not* a fair development of their powers, but a fair *attention* to their development. *I never got the whole jury to examine the glasses thoroughly.* After I had obtained from Mr. Beck a stand, Dr. Brooke of London, made the fullest trial with them which I could secure from any member, and he expressed himself favorably, though he has the natural national leaning of an Englishman. It would have been ridiculous for me to narrate all this in my report, but it is absurd for any one to interpret what I do say

as Dr. Hagen does." That effectually disposes of Dr. Hagen's inferences, that the American objectives "did not attain so much as others."

Dr. Hagen attempts to controvert the opinion now unanimously received in England and America, that the microscope should be so constructed as to receive an inclination. He says, "the statement made by people here that the working with high stand instruments (they being turned back) is much more convenient, as keeping the neck straight prevents the rush of blood to the head, makes rather a comical impression. I say comical, when we consider that for tens of years back several thousand low stand instruments have been in daily use in Europe without detrimental results." [?] Possibly no one but Dr. Hagen has ever heard that the use of vertical instruments caused a rush of blood to the head; but the experience of all microscopists here (Dr. Hagen excepted), is against the use of the low stand vertical instruments, and that evils and imperfect work do result from the use of such. To show that the "comicality" of the objection is not original with American microscopists, the following is extracted from Dr. Wm. B. Carpenter's work on the microscope, — an author whose opinion is certainly equal to Dr. Hagen's thirty years experience — written fifteen years ago. "Scarcely less important * * * is the capability of being placed in either a vertical or a horizontal position, or at any angle with the horizon, without deranging the adjustments of its parts to each other," * * * * "It is certainly a matter of surprise that opticians, *especially on the continent*, should have so long neglected the very simple means which are at present commonly employed in this country of giving an inclined position to microscopes, since it is now universally acknowledged that the vertical position is, of all that can be adopted, *the very worst*." Perhaps if Carpenter's work had been translated into German fifteen years ago it might not have been needful to write this paper.

Dr. Hagen has so little to say of American microscope makers, other than Tolles, that he found it impracticable to make so many mistakes in regard to them. If he had taken more "pains" he could have added materially to the number.

Of Spencer he says: "A few years ago, however, he retired from the business." This is a mistake, for which probably Dr. Hagen is not responsible. "I have not in fact had an opportunity to compare Spencer's objectives and oculars." "In Boston, Salem, and Massachusetts generally, there are none of Spencer's instruments to be found;" that is because he "spared the pains" to find them. The writer had them, and would have guided the enquirer to others.

Of Zentmeyer he remarks: "As near as I can find out he makes no glasses. Each of his stands that I saw had objectives and oculars of Tolles or Wales." Another example of the superficial knowledge obtained by Dr. Hagen; a portion of the very oculars which he saw on Mr. Bicknell's instrument, and which he gives the power of as Tolles, were made by Zentmeyer! Had he not "spared pains" to inquire, he could

have learned that Zentmeyer does make glasses, and that one of the Tolles' stands which he had seen was furnished with an excellent objective by Zentmeyer. In the notice of Zentmeyer's stand the most important and characteristic features are entirely unnoticed!

In his notice of Grunow's instruments he particularizes an inverted microscope, the peculiarity of which was a movement by friction rollers, an invention of Tolles, and which he (Hagen) had seen various modifications of on several of Tolles' instruments, in particular the first one in which it was ever introduced; yet he failed to notice it there.

It may, perhaps, be urged for Dr. Hagen that these things are trivial, and to some they may look so; but they constitute Dr. Hagen's paper; the aggregate of the trivialities makes about the whole. Dr. Hagen fails throughout all his papers to appreciate the difference between magnifying power and quality.

With a patronizing air that is "nearly comical," after reading the paper, he compliments the artists in these words: "Messrs. Tolles and Wales are no doubt artists of the first water, constantly endeavoring to advance and enlarge their science."

Dr. Hagen admits that he has not exhausted his subject, and promises to renew it; it is to be hoped that he will, and that when he does he will spare no pains to make himself thoroughly acquainted with it; if he endeavors to do that, all our microscopists will cheerfully assist him. — C. S.

ALASKA AND ITS RESOURCES.* It is not often that an exploration is able to show such results as Mr. Dall places before the public in this volume, even when assisted by public means. We cannot, therefore, praise too highly the modest manner in which the author tells us that he was unwilling to have the plans of the former director, Major Kennicott abandoned, and therefore, undertook to carry out the remainder of the explorations which were only half completed when the telegraph company abandoned the enterprise. The author was thus left alone for one year and succeeded in completing the survey of the Yukon Valley, unassisted except by the natives. As a thorough and reliable account of Alaska, with its pictures of subarctic nature, the substantial volume before us, with its beautiful illustrations, typography, paper and binding, will claim the highest rank and retain it for years to come. We feel proud of this elegant book, and that it is the fruits of American pluck, enthusiasm, and scientific zeal.

Many of the scientific results obtained by Mr. Dall have been already published in the *NATURALIST*, and the great value of his discoveries in a single department of zoology, *i.e.* that of ornithology, were passed in review in the last number by an able naturalist. In reading over the plain, unvarnished, modest narrative of personal adventure and explorations in Alaska, we are struck by the earnest endeavor of the author to

*By William H. Dall. Lee and Shepard, Boston, 1870. 8vo, pp. 627. With a map and numerous illustrations. \$7.50.